Responses to comments on the population model analysis, P. Wade 2/24/99.

MMC 8 January 1999.
Pg. 2, paragraphs 2-4. The comments regarding congressional intent, indicating that any population growth rate less than 4% should be considered a significant adverse impact, were interesting. We have chosen to estimate R_{max} from data from 1975-1991, and compare the observed growth rate from 1992-1998 to that estimated rate. However, there is also some merit to comparing an estimate of R_{max} from all the abundance data (1975-1998) to an assumed default value such as 4%. We will consider an alternative analysis along these lines.

IATTC February 16, 1999.
Pg 1, 4th para. The analysis in our report does take into account the precision and accuracy of the estimates in the standard statistical fashion. The precision of each abundance estimate or TVOD estimate is accounted for by its estimated standard error, which is used in the likelihood function. The accuracy of the TVOD is accounted for by an estimated parameter that scales the TVOD data to actual population numbers. It is not clear what "..consistent with a population growth rate of between 2% and 4%..." specifically means, in a statistical sense. If an analysis has been made, we can only comment on it if it is adequately described.

Pg 1, last P. Regarding the abundance estimates for northeastern spotted dolphins, the statement that "...the differences among them are several times larger than their standard errors." is perhaps true, depending upon how "several" is defined, but not unexpected. Roughly, for two estimates to be significantly different, they need to be the sum of 2 times both standard errors to be significantly different, meaning their confidence limits do not overlap. Given the possible trend in the population, it is most appropriate to calculate 95% confidence limits around a model trajectory and check how many estimates fall outside of these limits. To do this, we used the maximum likelihood fit of a generalized logistic model (using log-normal likelihoods) to the abundance and trend data through 1991, then calculated log-normal 95% confidence limits around this model trajectory using the relevant CV in each year. Of 9 RV abundance estimates from 1979 to 1990, only one falls outside such calculated limits, for a probability of 0.11, which is not that different than the expected probability of 0.05, especially given such a small sample of 9. In fact, with 9 trials with a binomial probability of 0.05, although 0 is the expected outcome, there is still a 0.37 probability of 1 or more occurrences. In other words, the observed outcome has a probability of 0.37, which does not make it an unusual occurrence. This indicates the differences can be explained by the estimated sampling error of the estimates. With respect to the precision of estimates of population trend, such estimates of precision will be calculated using standard statistical practices, and the precision of the estimates of trend can then be examined to judge whether they are adequate.

Pg. 2, 6th P. It is not clear that these factors would necessarily be greater in the earlier time periods. For example, the rationale for why misreporting would be greater in an earlier time period has not been stated. Given that vessels are currently operating under individual quotas, there could presumably be as much possibility of misreporting now as in any other time period — at the least it is not clear why misreporting would necessarily be less now. Regarding the statement that greater levels of stress from chase and encirclement would have occurred in an earlier period, it is not clear that the number of sets experienced by an individual dolphin would

have been greater in earlier years. The number of sets on dolphins has not changed substantially, whereas the dolphin populations have been estimated to be at perhaps 20% of their original size. In other words, at a constant number of sets, an individual dolphin would have an expectation of being set on 5 times more often. Furthermore, in the early days, a large percentage of dolphins set on were killed, which eliminated any possibility of stress over repeated sets having an effect. Now that mortality per set is lower, it may be that individual dolphins may experience more sets than they did in the 1960s. To conclude, an adequate analysis of possible trends in the expected number of sets an individual dolphin has experienced has not been undertaken to our knowledge, which makes it difficult to make conclusive statements regarding these factors at different time periods. It may be that an individual dolphin, on average, experiences more sets now than a dolphin did in the 1970's.

Pg 2, last P. We agree that using the term "unreported" was a semantic choice that was not necessary. The term has been modified to just state "additional mortality". A reduction in population growth must necessarily involve either additional mortality or fewer births; we have chosen to express it as additional mortality.

Pg. 3, last P. Regarding the 3 points made: (1) the analysis has been done with constant age-specific selectivities (not a fixed age composition — that was the method used in Wade (1994), but it is not used here). The selectivities are estimated by fitting to the observed age composition from aged animals 1974-1992, which covers nearly the entire period for which abundance data are available. No trends were noted in the age composition within that sample (S. Chivers, pers. comm, cited in Wade 1994). Additionally, no significant trend in the proportion of mature females in the kill was found for the northern stock (Chivers and Myrick 1993). Regarding the use of color stages in a "strategic analysis", we would certainly be interested in being made aware of published data on frequencies of color stages in the kill over time. Any reference to such work would be appreciated. (2) We would disagree that fishing effort needs to be used as an explanatory variable in the model. It is not clear how fishing effort would effect the actual population size, other than through direct mortality, which is already accounted for. As far as we can determine, the only other ways in which fishing effort could effect population size in a way not accounted for by the estimated kill would be through stress, cryptic kill, unreported kill, or some other such mechanism. Perhaps more details could be provided regarding how fishing effort improved the fit of a model. Additionally, it is our understanding that fishing effort is already accounted for in the TVOD data, so it would appear that adding fishing effort as an explanatory variable would be using fishing effort data twice, which seems inappropriate. (3) There is a misunderstanding. In analyses by NMFS, no population model has been fit to the pooled 86-90 estimates since Wade (1993).   Starting with Wade (1994), population models have only been fit to separate annual estimates of abundance. The current analysis continues that methodology.

IATTC February 3 1999.
None on modeling.

IATTC January 14, 1999.
Pg 2. 5th P. We agree that it was an error to use the term "Nmin". The statement should substitute the phrase "the population size"

Pg. 3. 1st P. Regarding the plausible alternatives, it would be helpful if relevant information were provided along with the hypotheses.
a). Our understanding is that a tuna management action has been taken (or is being considered) to exclude fishing from a large area which the northeastern spotted and spinner dolphin occupy. Presumably this is due to there being too few yellowfin tuna. This would argue against niche replacement. More information on the population status of billfishes would be required for us to comment further.
b). We are not clear how this constitutes a specific plausible alternative. We reiterate that the precision (and variance) of the estimates if fully incorporated into the estimates, and whether they are adequately estimated can be determined from examining the results. We feel that the precision of the estimates of the output quantities of interest are adequate. Furthermore, the decision analysis framework explicitly deals with issues of precision.
c). Natural variation in survival and reproduction must necessarily be relatively low in a long-lived mammal that is not capable of high population growth rates — otherwise populations would not be able to naturally persist. Such natural variation that there is will therefore be swamped by the amount of sampling error in the survey data.
d)
e)
f)
g) The assumed parameterization for density dependence covers a broad range of possible dynamics, as a wide prior distribution was specified for the parameter z that controls the density dependent response. Such models are an integral part of many fisheries stock assessments, and there use here is quite standard. Furthermore, the issue of a density-dependent model plays a relatively minor role in the analysis. We feel it is the most proper way to conduct the analysis, as it accounts for differences in the expected growth rate due to being at different depletion levels. However, we have also conducted the analysis in an alternative fashion which does not use a density-dependent model. That analysis gave similar results, indicating the results were not dependent upon the assumption of any particular form of density-dependent model. We would be happy to share the results of that analysis with you.
h.) It is interesting that the prior distributions have been questioned before we have reported what they are. We will do our best to respond to any specific comments we receive on the actual prior distributions that were used in the analyses.
i.) As the northeastern offshore spotted stock area and the eastern spinner stock area are entirely contained within the survey area, it is hard to understand how the survey could be biased from a re-distribution of animals during an El Nino. Furthermore, El Nino's are a regular occurrence in the ETP, and have occurred during previous surveys used in the trend analysis (i.e., 1993 and 1997).